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ABSTRACT

This examination of the topic of research in educational technology addresses four major areas: (1) why research is conducted in this area and the characteristics of that research; (2) the types of research questions that should or should not be addressed; (3) the most appropriate methodologies for finding answers to research questions; and (4) the characteristics of a research report that make it "good" and ultimately suitable for publication. The most important type of research for educational technologists to conduct is that which solves practical problems for contexts in which the complexity of the real world has not been controlled as it is in experimental research. Three aspects of this kind of research need to be considered: the development and validation of procedures for conducting decision-oriented research, methods of studying complex systems without having to dismember them, and the need to treat instructional development projects very seriously as case studies. Whatever methodology is used to conduct research, it must be applied with discipline and rigor. Given the eclectic nature of research questions in the discipline, a whole battery of methods--both qualitative and quantitative--should be used for investigation. Future trends in methodology in educational technology will follow the trends of educational research in general, i.e., more non-experimental studies will be conducted. Suggestions for successful publication of research efforts and a list of references complete the document. (JB)

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Emerging Trends in Educational Technology Research

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AECT Annual Convention

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The topic of this paper, or something very much like it, has become a fixed feature of the program of AECT conventions. Whether this means that the planners of each year's program do not pay attention to what has been presented on previous occasions, whether each attempt at addressing the question has failed, or whether there is a genuine and sincere concern that we constantly ask each other what researchers in educational technology should be doing, is not clear.

This year's examination of the issue attempts to answer four over-arching questions. The four questions are:

i. What is the nature of inquiry in Educational Technology?

ii. What do we need to find out?

iii. How do we go about it?

iv. What do we do with our findings?

Less cryptically, these questions point to a need to examine: why we do research in our area and what the characteristics of that research might be; the types of research questions we should and should not be asking; what are the most appropriate methodologies for finding answers to our research questions; and what are the

characteristics of a report of research that make it "good" and utlimately suitable for publication. Let me repeat, however, that a number of wiser heads than mine have tackled these questions before and have come to conclusions that are generally in agreement with those that I shall present. The reader might wish to consult these sources. (Torkelson, 1977; Salomon, 1978, 1979; Wilkinson, 1980; Clark, 1983; Gerlach, 1984; Heinich, 1984; and many others).

The Nature of Inquiry in Educational Technology.

In a recent address, Lee Shulman (1985) observed that arguments about the modes of inquiry that are common in the social sciences are almost totally absent in the "hard" sciences. By and large, physicists, chemists, astronomers and the like take methodology for granted. They agree about how research should be done. There are two reasons for this. The first is that the social sciences are younger than the natural sciences and have had less time to arrive at a consensus over methodology. The second is that the factors that social scientists study are more complex and less clear cut than those studied by their counterparts in the "hard" sciences.

Shulman's observation would be reason enough for beginning this paper with a look at the nature of inquiry in our discipline. But the matter does not end there. If one looks carefully at the literature, one is left with the impression that, with some notable exceptions, the matter of the nature of inquiry in our field has never been addressed adequately. We do not have a clearly-defined "philosophy of science" to guide us.

The exceptions will be familiar to most of you. They include some of the central ideas set down by Heinich (1970, 1984) about the nature of Educational Technology, the discussion of the role of prescriptive theory by Reigeluth (1983) and by Clark (1983). The issue is also addressed less directly by Jonassen (1984) in his philosophical analysis of mediation, Brody (1982), who describes a "functionalist" agenda for research on pictures, and Streibel (1985, & in press) in his critical analysis of educational computing. Notice that I have not listed any of the numerous papers at previous conferences dealing with naturalistic research methods. This does not mean that these papers have no value. They certainly do. But they do not delve deeply enough to define the nature of inquiry in our field.

The purpose of inquiry in any discipline is simply to find things out. We find things out for a number of reasons, not least among which is that we are by nature curious. This, of course, is a terrible truism, which is why we tend not to dwell upon the point when we initiate people into research. But the satisfaction of curiosity is supremely rewarding, and from this satisfaction comes the <u>enjoyment</u> one gets from doing research. It is sad that so many graduate students feel that doing research is drudgery, or worse, and that, largely because of this, they do no more once they have their dissertation out of the way. (Maybe, as Clark (1978) implied, this is also the attitude of their supervisors, in which case they are at a double disadvantage). But the fact remains that research is very satisfying, whether it leads to a significant contribution or not. Put another way, there is nothing wrong with doing research for its own sake.

Frequently our obsession with coming up with "significant" or "practical" results makes us devalue the work that we really enjoy, and turns us away from doing more.

The curiosity that inquiry satisfies is curiosity about why things are the way they are, and why they behave the way they do. Research therefore becomes a quest for explanations. In our area, this might involve explanations of why one instructional method succeeds where another does not, why some delivery technologies are thought of as more reliable or easier to learn from than others, why administrators will spend money on some technologies and not others, why smart students learn better from some instructional methods than less able students, and so on. The explanations that researchers find for such phenomena will, one hopes, fit together in non-contradictory ways into a body of theory which, in turn, will direct us towards what explanations to seek after next. This is the familiar cycle of research building theory from which research hypotheses are derived, and is described in any research methods text. The result of this type of inquiry is descriptive theory that is typical of the natural sciences (Simon, 1981).

A number of scholars in our field find even greater satisfaction when the explanations they find are applicable to practical problems. In these cases, the purpose of research is the generation of prescriptive theory. It is this type of theory that guides the decisions that instructional designers make, a process that is the pivot around which the whole field of Educational Technology turns. As Reigeluth (1983) has stated,

prescriptive instructional theory consists of an integrated set of principles that state which methods to use given certain outcomes to attain and conditions under which instruction is to occur. Simply put, these principles take the form: "If x and y then do z". For example, if you want students to be able to solve word problems involving fractions (outcome), and if the students know how to perform the four basic arithmetic operations on fractions and have high fluid ability (conditions), prescriptive theory might point to using an inductive method with lots of feedback. A number of scholars in our field have drawn together bodies of prescriptive theory from research that are of value to instructional designers. These include all of the authors in Reigeluth's (1983) volume, the work of Fleming and Levie (1978), and the prescriptions for designing concept lessons offered by Merrill and Tennyson (1977).

Inquiry in our field, then, serves three purposes: it satisfies our curiosity about the way things are; it leads to the discovery of explanations about what happens when people learn, interact with technologies, and so on; and it allows the development of prescriptive theory that can inform decision-making. It does, however, have one other important characteristic, which Shulman (1981) described as having method or orderliness. Regardless of which particular research paradigm you subscribe to -- experimental, historical, ethnomethodological, phenomenological, evaluation -- all inquiry that is worth anything is rigorous. What this means varies from paradigm to paradigm. However, scholars of every kind should be able to point to examples rigorous of inquiry within their

specializations. It is terribly wrong to think of one paradigm as being necessarily better, than another. But, as we all know, some experimental researchers look down upon naturalistic inquiry as somehow less rigorous and inferior, while some naturalistic researchers scorn experimental research for lacking applicability. Such divisive squabbles are counterproductive and unnecessary. It is far better to accept different standards of rigor (see, for example, Guba, 1981) and acknowledge that all types of good research make a contribution to our field.

At the risk of belaboring the point, I would like to take this argument one step further. Increasingly, we read that prescriptive theory and design are unnecessary, and that we should rely upon the expertise of teachers and other professionals "in the field" to make instructional decisions (see Nunan, 1983). However, the same standards of rigor must apply here. The ability of expert professionals to "reflect in action" (Schon, 1983) must not be thought of as a pretext for sloppiness. Decisions based upon experience, intuition, hunches and "plausible reasoning" (Hunt, 1982) need to be just as sound and carefully deliberated as decisions based upon a more formal prescriptive theory.

What do we need to find out?

A number of scholars have reviewed the progress of research in Educational Technology over the years, and have been able, from this historical perspective, to point to the gaps in what we know (Torkelson, 1977; Clark, 1983; Gerlach, 1984). This approach has

also led to the conclusion that we have not been asking the right research questions, that we have not been trying to find out what it is really important to know.

The development of research in our area is generally acknowledged to have followed the following stages:

1. Media comparison studies, in which the effectiveness of one medium is compared to that of another medium or to "traditional" classroom instruction. Clark (1983), among others, has demonstrated convincingly that this research is a complete waste of time because there is nothing intrinsic in the media by which instruction is delivered that can possibly affect how and what people learn.

2. Media attribute studies. Here, researchers attempted to identify specific attributes of the different media which made them unique and which might account for differences in outcomes when different media were used to teach. Again, there are conceptual and methodological difficulties with studies of this kind which preclude their contributing much of value to theory.

3. Aptitude-Treatment Interaction research. Snow and Salomon (1968) pointed out a long time ago that differences in student aptitude and ability would "mask out" main effects in media research, and that they should be included as factors in research designs in our field. A great many studies have now been conducted using the ATI paradigm, often claiming to find interactions, although, again, there are many methodological problems that make the majority of these studies suspect (Cronbach and Snow, 1977).

4. Learning strategies research. While not within the

"mainstream" of educational technology research, a great deal of work is now being conducted on the strategies students use to learn (Brown, 1981; Brown, Campione and Day, 1981). Just because a student has ability in a particular cognitive skill does not mean that the ability will be used to learn when it is appropriate to do so. The use of strategies by students is an important area for researchers in our field to address.

5. Research on students' attributions. Salomon (1981, 1982, 1983, 1984) and Clark (1983) both state that the importance of a particular instructional technology derives from the value and difficulty attributed to learning from it by students. Again, this research is not yet thought of as "mainstream" educational technology research. However, it points towards an important future development.

This evolution of research illustrates both the good and the bad, the productive and the less productive, in what we have done in the past. Generally, the more recent research is more valid and describes things more accurately, (although there are still people doing media comparison studies, in spite of the fact that we know they are a waste of time). Also, this type of research is conclusion oriented, and represents only part of what educational technologists do. We have a considerable tradition of decision oriented research, which is conducted under the rubric of "Instructional Development". From our past research, we have learned some useful things about how student characteristics are related to different instructional methods, though this information is too fragmented to qualify as an instructional

theory. We are beginning to discover the relationships among task, student characteristics, methods and strategies that students actually use. We have a clearer, though by no means complete picture of the cognitive processes that underlie learning associated with memory, attention, recall, imagery, problem solving, and so on. On the other hand, for all our efforts, we have no theory that relates media or technologies to learning (nor, some might say, is such a theory potentially interesting nor even possible). We have very poor procedures for making instructional decisions based upon the most recent cognitive research. All of the design models I can think of are built upon behavioral premises and principles. Such prescriptive theory as we have is so incomplete as to appear incoherent at times, and is made up of instructional principles that might have "local" validity, but which, by and large, are not particularly robust or generalizable.

From this, it is possible to identify some things we should be finding out. Whether these will develop into future trends or remain pious hopes remains to be seen. We shall look first at two general research areas, and then at several more specific types of question.

Prescriptive Instructional Theory.

We must continue to conduct conclusion-oriented research to uncover prescriptive instructional principles which will fit together in a coherent and stable theory. In spite of a great clamor to the contrary (Phillips, 1983; Eisner, 1984), there is still a need in education for both principles derived from experimental research and heuristics derived through qualitative

modes of inquiry (see Howe, 1985). Indeed, there is increasing evidence from Philosophy (Schon, 1983), Cognitive Science (Hunt, 1982) and Artifical Intelligence (Feigenbaum, 1985) that both experimentally verifiable factual knowledge and intuitive tacit knowledge are necessary for the development of the levels of expertise required for the successful execution of higher cognitive processes. An "expert" uses experience and intuition to fill in what theoretical principles do not deal with. Likewise, an expert system relies on a knowledge base, derived from public knowledge, and a set of heuristics, derived from human experts, in order to function.

Our research must therefore attempt to discover, verify and establish as complete a prescriptive theory of instruction as possible, on the understanding that its inevitable incompleteness can be made up for by capturing the heuristic knowledge of experts. This research requires that we continue to seek knowledge about the factors that are necessary, not merely sufficient, to bring learning about (Clark, 1983). In practical terms, this requires the study of basic cognitive processes, human abilities, learning strategies, and attributions of learners' successes and failures to the various methods and technologies we employ when we teach. These factors are highly interdependent, and deserve study in contexts where that interdependence is not destroyed, and even becomes the object of the study itself.

Decision Oriented Research.

Heinich (1984) has made the point that the most important

type of research for educational technologists to conduct is that which leads to solutions to practical problems within contexts in which the complexity of the "real world" has not been "controlled for" as it is in experimental research. The emphasis is on the application of knowledge to solve problems, and loudly echos the definition of "Technology" upon which our field is built (Galbraith, 1969).

There are, I believe, three aspects to this kind of research that need to be considered. The first of these is the development and validation of procedures for conducting decision-oriented research. The procedures that we have to do this -- our instructional development and design procedures or "models" -- have by and large been developed intuitively. As Gerlach (1984) has pointed out, with rare exceptions, they have not been submitted to the kinds of empirical testing that we require of the postulates of theory. Gerlach would have us study the behavior of educational technolgists themselves, requiring that design models become independent variables in research studies. In this way, we would be able to ascertain whether, and which, design procedures result, ultimately, in superior student performance. We have not yet answered the research question: does following a design or development model lead to superior instruction?

Second, we need to develop ways of studying complex systems without having to dismember them. Precisely what needs to be done in this regard is less clear. However, the concepts and methodologies of General System Theory (Bertalanffy, 1968) would serve as a good place to start. By and large, I believe that

educational technologists have completely missed the point of System Theory, and have confused being "systemic" with being "systematic" (Davies, 1981). The so-called "system approach" has resulted in little more than "models" of linear procedures which, if followed, lead from the statement of a goal to the creation of a unit of instruction. The emphasis is heavily biased towards "front-end" analysis and field testing, with little guidance being given at the decision-making stage where methods are selected to match the conditions and outcomes that the analysis has clarified. The real purpose of System Theory, however, was to provide ways of managing great complexity, originally in living organisms. The decisions through which instructional methods are selected require that we deal with such great complexity. Students are very complex, and the multivariate nature of learning and instruction simply cannot be dealt with by our terribly naive "system approach". What we need to find out is how the ways for dealing with complexity, suggested by those few educational technologists who have really understood System Theory, can be applied in practice to the making sound, though complex, instructional decisions. The work of Heinich (1970) on the complexities of the management of instruction, of Pask (1984) on self-organization in complex systems involving students and instructors, and of Winn (1975) and Beckwith (1984) on open system models of learners, is instructive in this regard.

Finally, we need to treat ID projects very seriously as case studies. Obviously, the primary purpose of an ID project is to get a problem solved as efficiently and effectively as possible.

However, we lose a great deal of valuable information every time that those in charge of a project fail to describe it in the literature. I have argued elsewhere (Winn, 1981) that ID is a valid and valuable form of research. By the standards that are applied to experimental research, ID falls short when it comes to generalizability of findings. Because ID projects set out to solve local one-shot problems, and because few variables if any are controlled, one might argue that the experiences would be so specific to that one case that they could not possibly be of interest to other developers. But it is a question of whether the mug is half empty or half full. Looking at it the other way around, there are certain common elements in <u>every</u> ID project: there is a "client", a need, a goal, a set of objectives, a solution, try-out and implementation. How each of these is handled differently under different circumstances is very instructive, especially when one considers that the developer seeking "methods" which might help in a particular project will certainly be intelligent enough to adapt the suggestions of a fellow developer to fit his own needs more precisely. The case study seems to be the best way to accomplish this, and educational technologists should be encouraged to use it.

The following specific research needs and questions are not presented in any particular order. They encompass both conclusion- and decision-oriented research.

<u>Metacognition</u>.

There is increasing evidence that students are capable of monitoring their own cognitive performances and of making deliberate changes in the strategies they use to learn (Brown,

1981; Brown, Campione and Day, 1981). This is generally seen as something to be desired, for it allows students control over what and how they learn. There is, however, a Catch-22 as far as educational technologists are concerned. It involves the question of who knows best, the designer-instructor or the student? For example, if an instructional designer chooses a particular instructional method on the basis of prescriptive theory, expertise, or what has worked in the past, then it is assumed that the method is indeed the best, given the circumstances. Now, if the student monitors his performance and decides that another method would be better, who is right? On the one hand one might argue that if students "interfere" with what an "expert" has decided is best, then design is a waste of time. On the other hand, if students do indeed know more about what is good for them than designers can possibly know on the basis of their meagre learner analysis, then design helps students take the intermediate step between complete novicehood and mastery of learning strategies. In either case, design becomes unnecessary. But in the latter instance, the reasons for this are positive.

What comes out of this conundrum is the apparent need for research into the relationships among prescription and learner control of instructional methods. Some work has already been carried out on this problem (Carrier, Williams and Davidson, 1985; Tennyson, Christensen, Walsh and Hajovy, 1985) in the study of computer-assisted learning. However, we need to find out whether the prescription of methods by designers or instructors is antithetical to or facilitative of metacognitive skill in

students. In this way it should be possible to identify at what point a student should be allowed to make metacognitive decisions rather than abide by prescribed ones.

Perceptions of Media and Technologies.

The work of Salomon (1981, 1982, 1983, 1984) on attributions of qualities to media by students has opened up a new area that educational technologists should study. The suggestion is that the effects and effectiveness of different media and delivery technologies are determined mostly by how the students perceive them. This includes whether the medium is considered to be "easy" or "hard", whether the student thinks it is worthwhile putting any effort into it, and so on. In addition to Salomon's work, Krendl has studied the effect of "set" (Krendl and Watkins, 1983) and "preconceptions" (Krendl, 1985) on television viewing. Salomon (1985) has turned his attention towards similar questions involving computers. These studies serve as models for others to follow.

Technological Constraints.

While it has been established (Clark, 1983) that it is instructional methods that have an influence on learning rather than media or delivery systems, the efficiency with which methods can be used is frequently constrained by the technologies available for the delivery of instruction. Thus, while Clark has claimed that a teacher can use any method that a technological medium can, it might not be very efficient for the teacher to do so (see Petkovitch and Tennyson, 1985). For example, it is theoretically possible for a teacher to have 54000 overhead transparencies on file in a classroom, which the teacher selects

from as needed. However, to do this is impractical, and far less efficient than using a videodisk for the same purpose. Likewise, it is far less practical to have students commute hundreds of miles to class each week than to have them study pre-packaged self-instructional materials in their homes.

We therefore need to find out about the efficiency of various combinations of instructional methods and delivery technologies in order to add a practical dimension to prescriptive theory concerning instructional methods. A recent study by Wager (1986) appears, at first glance, to be yet another media comparison study. In it he compares CAI and television for teaching army teletype operators. However, he was not interested in whether one technology taught the soldiers more or better than the other. His concern was in cost effectiveness, and a large part of his data consists of detailed costing out of teaching via the two technologies. This is a very useful type of study to conduct, and we need to do more of them with a variety of methods and delivery systems.

Optimization Methods.

The roots of instructional design, some claim (Glaser, 1976), lie in Simon's book The Sciences of the Artificial (Simon, 1969). Here, Simon describes design as a process whereby decisions are optimized on the basis of the demands of a task and the constraints that are imposed upon its performance. Optimization procedures, such as statistical decision theory, work very well in some sciences, like engineering and maybe medicine. However, as Glaser has mentioned (1976), they only work

in Education in "trivial" instances. This is because, as we saw above, education is concerned with complex, multivariate processes that we do not yet fully understand, let alone control. However, this does not mean that more precise procedures for instructional decision-making should not be sought after. The speed and capacity of today's computers makes it possible to handle those very complex multivariate situations that instructional decision-making is involved with. Some work has begun on this problem, again in CAI research. Tennyson and his colleagues (Johansen and Tennyson, 1983) have developed the Minnesota Adaptive Instructional System (MAIS) which has some of the attributes of a complex instructional decision-maker. The algorithms involve, among other things, the calculation of of probabilities using Bayesian statistics. We need other mathematical models of this type to help us optimize instructional decision-making.

"Expert" Design Systems.

Everyone is talking about "expert systems". These are computer programs in which has been captured the intuitive tacit knowledge of experts, expressed as decision rules through the agency of "knowledge engineers". We need to find out whether it is feasible and practical to capture the expertise of instructional designers in this way and create expert systems that do instructional design.

This would be relatively easy to do. But the next logical step is less simple. If we can create expert instructional design systems, it should be possible to create CAI systems that design themselves as they interact with students. In other words, the

prescriptive principles embodied in an instructional theory would be "invented" by the system as it "got to know" each student it was teaching. In effect, a separate theory of instruction would develop for each student, offering the ultimate in adaptive instruction.

Significant steps have already been taken towards constructing these self-designing systems. They are to be found in self-improving intelligent tutoring systems, developed by such researchers as Kimball (1982) and O'Shea (1982). These systems function by changing instructional strategies, either on the basis of how well the student is doing or arbitrarily at predetermined intervals, and seeing whether or not the change in strategy improves or degrades the student's performance. The algorithm is, once again, based upon probability. Based upon the success of the strategy, it is either retained or changed again. We need to find out how effective such self-improving systems are, and to develop more sophisticated algorithms for the modification and retention of instructional strategies.

Presentation Formats.

With the advent of intelligent tutoring systems (Sleeman and Brown, 1982) comes the need for developing a whole variety of ways of displaying information to students. To date, the natural languages that have been used in intelligent CAI have been verbal langauges, like English, French and Russian. However, research on the effectiveness of graphics (Winn, 1986), pictures (Levin, Anglin and Carney, 1986) and videotex (Winn, Ellis, Plattor, Sinkey and Potter, 1985) in instruction has firmly established

that other ways of presenting information are now as feasible to use as text, and are often more effective. The problem is that in order to get information from a knowledge base in a computer, be it constructed from decision rules, a network of propositions or one of many logics, into a form that the student can comprehend requires routines that themselves follow particular sets of rules. In the case of verbal language, these rules are the language's syntax. There is no syntax for the non-textual representation of knowledge. A number of scholars have attempted to describe grammars of pictures and graphics (Doblin, 1980; Szlichcinski, 1980). However, we simply do not know enough about how pictures and graphics convey meaning to enable us to write decision rules for CAI with the same power and precision as the rules we have when screen presentations are textual. There is a tremendous amount of research to do in this area before we can begin to exploit these forms of representation to their fullest.

There are, of course, other things we need to find out. But any report of "future trends" must limit itself to the most likely to be manageable. I believe that the items just mentioned are pretty "sure bets" for the future of research in our field.

Methodology

A section on methodology is expected in a paper such as this. However, given the plethora of papers and articles on the methodology of educational research that have appeared recently, what I have to say on the matter will probably be redundant. I will therefore be brief.

First we must remind ourselves that, whatever method we use

to conduct our research, we must apply it with discipline and rigor. The arguments in support of this contention were made earlier, besides which they should be obvious.

Second, let us not forget that our discipline is concerned with both conclusion- and decision-oriented research. The methodologies for each differ, as we all know. Again, recall the arguments made above that instructional development is a form of research, and that the data instructional developers gather should be reported.

As far as the current debate about qualitative versus quantitative methods is concerned (Phillips, 1983; Eisner, 1983; Smith, 1983; Miles and Huberman, 1984), I am bothered by the feeling I get that proponents of each class of method are proposing alternatives to rather than complements to the other class of method. Given the eclectic nature of research questions in our discipline (and in Education generally), we must have at our displosal a whole battery of methods to deal with the different types of things we need to find out.

There are a number of ways in which experimental/quantitative and naturalistic/qualitative methodologies can complement each other. Howe (1985) has stated that "quantification extends, refines and crosschecks qualitative knowledge" (p. 10). On the other hand, it is now common for experimental researchers to do what Bronfenbrenner (1976) suggested ten years ago to improve the ecological validity of controlled experiments: talk to the subjects afterwards about what they learned, how they did it, what they thought of the

experiment, and so on. So we should try to use quantification, where appropriate, to sharpen our interpretation of qualitative data, and to use what we might call "post-experimental debriefing" of subjects in controlled experiments to find out more about the subjects and how they learned.

The work of Winn and Everett (1979) and McIsaac, Mosley and Story (1984) illustrates one way to do the former. In these studies, factor analysis and multidimensional scaling were applied to subjective ratings of pictures in order to make them more interpretable. This type of study is, of course, "reactionary" in the sense that it still purports to enjoy a measure of experimental control. However, it is not a big step from there to what Howe suggested -- the application of relevant quantitative techniques to data gathered through observation.

To date, I have observed that educational technology researchers have spent far more time telling us why we should be using naturalistic methods than they have doing naturalistic studies. Maybe this is because we are still a little bit afraid that these studies are less likely to be published. Or maybe it betrays our "behavioral" roots and the accompanying belief that all of the behavior that is of interest is amenable to quantification. Be that as it may, there is certainly not the activity in educational technology research as there is in research in teaching and learning in classrooms, for example.

However, I believe that naturalistic inquiry has just as important a role to play in Educational Technology research as it does in other areas. A study by Eastman (in press) serves as a model of the kind of work I have in mind. Eastman gathered data

by observing students working with comouters and computer printouts in a language arts class over a period of several months. Her application of the techniques of naturalistic inquiry (see Guba, 1981) to safeguard the validity of the results is exemplary. Her results shed much light on how students think of computers, and how this affects the ways in which they use them. And the type of information she reports could not have been obtained in an experimental study.

In sum, then, I believe that the future trends in methodology in Educational Technology will generally follow the trends in Education as a whole. That is to say, we shall see more non-experimental studies being conducted. However, I feel that our area is probably more reactionary in switching to this type of methodology than other areas in Education because we more than many other scholars have our roots in the observable, quantifiable phenomena so popular with behaviorists. However, we need to develop ways of bringing naturalistic methods to bear on research questions of interest to us, and especially to develop techniques in which qualitative and quantitative methodologies work together rather than against each other in the gathering of data.

Publication of Results.

Once again, so much has been said about how to get research published that I will be brief to avoid redundancy. There are, of course, a few, obvious, general rules. Following these by no means guarantees publication. But failing to follow them does guarantee that the paper will not be published.

First, make sure that you follow the style manual specified in the journal's "Guidelines for authors". Even established scholars often fail to do this, and editors find it most irritating.

Second, make sure that the topic and type of paper are suitable to the journal. You would be surprized if you knew how many manuscripts submitted to ECTJ are about topics that have nothing to do with Educational Technology, however broadly you define it. Also, send scholarly papers to scholarly journals and less scholarly ones to journals that publish non-scholarly articles. I reject many good papers because they are not scholarly. Often these end up in other journals, as they should. But sometimes authors are discouraged by a rejection and do not send their papers to a second, more suitable, journal. This means that a good article that has something to say never does get into press, and we are all the poorer for it.

Make sure that you meet all of the journal's technical requirements. If they ask for four copies, send four copies. If it is to be typed on 8 1/2 by 11 paper, type it that way. Never send little notes to editors in which you explain why you did not follow the technical or stylistic requirements set out by the

journal.

The last matter is of more substance, and in a sense recapitulates a lot of what has been said in this paper. Make sure that what you are reporting or writing about will be of interest to the readers of the journal, and will be respected as sound scholarship. This has requirements that concern both the question that you write about and your methodology.

As far as the first point is concerned, you should pick research topics that contributes to current knowldedge, not to something that ceased to be of interest and importance years ago. Clearly, media comparisons are no longer worth publishing, nor are a number of other topics. In order to meet this requirement, you must keep up with what is going on in the field. All of us who supervise graduate students have had to deal with the student who is terribly excited about a particular research question or aspect of theory, not realizing that the question has already been answered, or that the piece of theory belongs to a body of knowledge that is no longer considered to be valid or valuable. It is sometimes hard to persuade such students that their proposed study has no value, and if completed, will contribute nothing and will certainly not be published. So, stay current and read as much as you can.

The same applies to your methodology. Do not use designs that have been superseded by more sophisticated and robust ones. And do not use techniques of analysis that are no longer considered to be acceptable. A frequent mistake, for example, is to use multiple t-tests instead of range tests when a significant difference has been found by analysis of variance with more than

two groups. Similarly, do not use parametric tests on ordinal data. That is another frequent flaw in manuscripts that I see. There are a number of these pitfalls, and you should be vigilant and knowledgeable enough to avoid them.

You must also make sure that you have something of consequence to say. Your ideas may be current and the methodology impeccable. But if the results of your empirical study, or the arguments you make in a position paper leave the reader with a "So what?" feeling, then you have not really presented anything that is worth publishing. (After reviewing one such study, an ECTJ reviewer wrote, "What a waste of subjects!" To me, that says it all.)

Finally, a publishable manuscript has to have an internal logic to it. By this, I mean that the hypotheses of an experimental study must be clearly derived from the literature review; that the design must allow one to test the hypotheses; that the techniques used to analyze the data must be appropriate to the design; and that the conclusions must follow from the results. There is nothing worse than hypotheses that appear out of the blue, apparently at the whim of the author, except perhaps for conclusions that are equally unattached to anything that has preceded them. The same spirit should also be present in reports of non-experimental research. The arguments should flow well and the points should follow one another with an obvious logic.

Conclusion

In conclusion let me just remark on the impossibility of

speculating with any accuracy about "future trends", whether they be in Physics, Literature or Education. One is tempted to take liberties, as I believe Frank Ogden did recently (Ogden, 1985) when he predicted that, if current trends in "high tech." continue, people will marry robots. I have tried not to take such liberties, but rather reflect upon what I see beginning to happen in research in our field and in kindred areas. I believe that the research questions that I have suggested we need to answer, and the methods we need to develop and use to answer them, do anticipate quite accurately directions in which the field will move. And I hope that the simple suggestions for preparing publishable manuscripts will be of some assistance in getting answers to our research questions into the literature for all to share.

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